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Willard - Circulation by Respiration

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THEORY
OF
Circulation by Respiration.

SYNOPSIS OF ITS PRINCIPLES AND HISTORY.

WRITTEN, BY REQUEST,
FOR THE "U. S. JOURNAL OF HOMŒOPATHY,"
BY EMMA WILLARD.

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1861.

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THEORY

OF

CIRCULATION BY RESPIRATION.

SECTION I.

First step in the discovery—Animal Heat the product of Respiration. Second step—Heat evolved in the lungs by Respiration there produces Expansion. Third step—Expansion; implied motion, which from the organism must conduct the blood to the left ventricle of the Heart. Theory imperfect, until the formation of sufficient vapor or steam in the lungs is perceived and acknowledged.

TO DR. MARCY.—In complying with your request to write for your journal an article embodying my theory of the motive powers which produce the circulation of the blood, together with some account of its rise and progress, I obey what I regard as a call of duty; and thus requested, do it with pleasure.

But my theory, with its history, cannot thus be written without egotism. Logicians say, that the way to convince others is to retrace, in order, the steps by which you yourself became convinced, which is to be egotistic. But in this case, there is a further reason: the scientific discoverer must speak of the apparatus by which he experiments, and mine was often my own physical frame.

Twenty years ago, while yet my mind, laboring with this great subject, was condemned

“to drudge
“ Without a second and without a judge,”

you, sir, comprehended the hypothesis which has now become a theory, and you waited not for others to speak, but you fully acknowledged its truth; and although, in Hartford, as now in New-York, you were thronged with practice, (then allopathic), you yet found time to furnish me with added experiments, made in your office, confirmatory of its truth, which by your permission were afterwards added in your name to my published work.

The first step in the theory occurred to my mind in the winter of 1822, and while I was engaged in founding the Troy Female Seminary. Being in attendance on a course of lectures on chemistry, and at the same time teaching to a class Mrs. Marcett's excellent work on that subject, one cold morning, as I was walking briskly up a hill, I said to myself, Why do I grow warm? Whence comes this accession of caloric? It cannot be transmitted to me from any object without, because every thing which comes in contact with

me is cold. Snow is under my feet, and frosty air surrounds me; and, as to clothing, even the softest furs *impart* no warmth—they but keep from escaping that which comes from within. What other method besides transmission is there of gaining heat? There is the elimination of caloric, when, in substances chemically combining, weight is gained and bulk is lost. Is there any such combination going on in me? Yes; this atmospheric air, when I inspire it, has oxygen combined with nitrogen; but when I expire, the oxygen has disappeared, and heavier substances—carbonic acid gas and watery vapor—are returned in its place. Thus, it must be, animal heat is evolved. It is the product of respiration; and it is because I breathe faster and deeper, that more carbon is oxidized or burned, and more heat is set free in my lungs; and therefore I grow warm as I walk up this hill, though all around me is cold.

The mind, excited by new and great thoughts, works with unwonted energy; and mine at once collected so many proofs, that I became perfectly convinced of the truth of the hypothesis. In searching books, I found that Lavoisier had taught the same; but he dying, his doctrine was discarded by English chemists, Dr. Black leading the way, and therefore it did not then appear in English systems of chemistry. But from that time, I cherished it with a mother's devotion, watched changes in my own physical frame relating to it, taught it to my pupils, and held warm disputes with the medical faculty, who opposed and contemned it.

In the summer of 1832, the Asiatic cholera appeared among us, appalling every heart. This plague, I said, is a disease of coldness and obstruction; and these doctors, wrong as they are on the subject of animal heat, can never understand it—though, if Lavoisier were living, he might. Let me, then, as best I may, consider anew the problem of heat as produced by respiration, and see whether I cannot find out something which has a bearing on the fatal coldness of this fearful disease. It is into the lungs, and nowhere else, that breathing introduces atmospheric air; and it is there that the oxidation of carbon or animal combustion takes place. Thus must caloric be imparted to the blood in the lungs; and in them is one-fifth of the blood of the system, of which seven-eighths is water.

The nature of heat is to expand all fluids. The blood in the lungs must, therefore, expand; and if it expands, it must move; and if it moves, it must, from the organism of the parts, move to the left ventricle of the heart, into which the valvular system opens to give it a free passage—whereas the valves of the right close against it. "Eureka!" I mentally exclaimed; "I have found the *primum mobile* of the circulation of the blood." I had for years disbelieved that the heart's slight mechanical impulse was that cause. In teaching Paley's "Natural Theology," my mind had come in contact with the passage in which he describes the heart's more than Herculean labors; and I said, "This is altogether too much—the heart alone cannot perform all this—there must be some other power;" and an abiding desire to know what

that power could be, prepared me for receiving this great idea. But my mind was agitated by it, as the sea is, when a great rock is thrown into its waters.

The cholera was then raging around me; and as I prepared to flee from it to a mountain air, I confided to a scientific friend, Professor Twiss of West Point, my hypothesis, which I regarded as probably the incipient germ of an important discovery.

But there was first the former theory to be disproved; and then there were new points to be investigated and established. In the ensuing winters of 1833, 4, and 5, I gave much attention to the subject, and employed professors in my school in the departments of chemistry and natural philosophy, who assisted me,—particularly by their ingenuity in the construction of such simple pieces of apparatus as were needed.

Thus we proved that, although the heart's action gives pulsation, it does not necessarily give circulation. By an endless india-rubber tube, filled with water, coiled upon a table and struck repeatedly at one point, a pulsation was produced throughout, but no circulation. By affixing the tube to a vessel of water, and laying it on an inclined plane, the water ran through it in an equable current, making circulation without pulsation. Clapping the hand upon the tube in successive contractions, the fluid passed on *per saltem*, producing circulation and pulsation united, but no acceleration of the current. Now, add valves to the tube on each side of the opening hand, and you will have the current—which is moving by gravitation, accelerated by the hand's impulse, as the blood's current, first moved by respiration, undoubtedly is by the heart's beat.

The heart we regard as the grand regulator of the blood's flow; and it is admirably situated for measuring out a regular portion of blood at every contraction. John Bell, believing in the Harveian theory, said, "It is awful to think of the unfixed position of the heart;" and Dr. Arnott declared that "the heart, the heart alone, is the rugged anomaly in the laws of fitness in mechanics." The heart was now seen to have a right position; for it should swing loose that its moorings be not endangered; and, as whatever impugns the Creator's unerring wisdom must be wrong, so the presumption is, that whatever vindicates it must be right.

My hypothesis assumed the principle, that, if an endless hollow tube be filled with a liquid, the liquid can be made to circulate perpetually, if it be heated at one point and cooled before its return. A drawing of the simple apparatus by which this problem was proved, is given in my published work on "the Motive Powers, &c." The figure which represents this apparatus gives the learner the most simple idea possible of the connection of the respiratory and circulatory systems, and of the combination of the two motive powers; the first, or chemical, coming from the lungs, and the second, or mechanical, from the heart.

Suppose the heart divided into right and left hearts by dissection at the

septum: the circulatory system might then be represented by an endless tube. Let such an one, nine or ten feet in length, and of one inch bore (to be filled with water) be placed upon a horizontal table. Let an enlargement of the tube be made by a tin vessel to represent the lungs, which shall contain about one-fifth part of the water. Let the tube connected with the right side of the vessel have, at a little distance from the vessel, a smaller enlargement, composed of india-rubber, which can be grasped by the hand, to represent the heart's right ventricle, with a valve on each side opening towards the tin vessel, the two to represent the tricuspid and semi-lunar valves. Let the whole be made nearly full of water; then, under the tin vessel (representing the lungs), let a fire be made. As the water heats, it will expand; and as the valve closes to the right, it will go off to the left side of the vessel. But, as no water will come in from the right, on account of the valves, there will be no current. Now let the hand grasp the india-rubber, and the fluid between the valves being displaced by its pressure, all the water will go towards the tin vessel, because, while the valve representing the tricuspid would close, that representing the semi-lunar (between the mimic heart and lungs) would open—and very freely; because the expansion made by the heat under the tin vessel had created a vacuum, and thus made a suction power to draw it forward, while there is a driving power behind to force it onward into the tin vessel. Then relaxing the hand, a vacuum will exist between the two valves; and the valve in the rear of the current now begun (the tricuspid) would open, and the water rush in to fill the vacuum in the india-rubber ventricle, to be again pressed forward by the next grasp of the hand; and thus—the fire (representing respiration) being kept up, and the alternate grasping and relaxing of the hand (representing the heart's regular impulse)—a perpetual circulation might be made to go on;* but not without another condition of the problem.

And it was in performing this experiment that a truth was discovered, which, had it been known, many who have ignorantly lost their lives might have preserved them. When the fluid in the apparatus became equally, or nearly as much, heated at the extreme parts of the circulating tube as at the heating vessel, then the motive power of expansion ceased, and (the hand's impulse being too weak of itself to carry it on) circulation failed; but it was

* It is here seen what an important work this theory does for the venous circulation, and why the blood moves into the lungs. We have read of a theory which maintains that it goes there because there is a mutual attraction between it and the capillaries of the lungs. But there is none between the water in our tube and that in the tin vessel where water is boiling; but it goes into it with a rush notwithstanding. Because there is a strong suction power produced by expansion, no other attraction is needed. The apparatus, as here described, goes no farther than to represent the circulation in single-hearted animals. But in my work is a drawing which shows the left heart on the opposite of the mimic lungs from the right; and then how the same tube, by being folded in the form of a figure eight (8), shows the two hearts united into one, and both ventricles working by the same contractions to perform their different tasks.

restored by putting snow or ice around the extreme parts of the tube. How often have we heard of ladies who, having gone into warm baths, have been found dead by their friends, or too nearly so, to be restored.* Through ignorance of the cause, no right means would be taken to restore them, such as dashing cold water upon the exterior, with simultaneous efforts to produce, in fresh air and in proper position, such artificial respiration as leads to the natural. Where no internal lesions have occurred, there is every reason to believe that such measures might produce restoration.

My imperfect machines gave me to see how much might be done for this important part of physiology by a more perfect apparatus. Mine was merely horizontal—but one might be made to take as many positions as are natural to the human frame; and how many facts might such an one elicit concerning the effects of position on the circulation, by which lives might every day be saved! But skilful mechanicians, not ordinary mechanics, are needed, who are men of intellectual capacity, and are furnished with *carte-blanche* for time and expense.†

The years 1836-'7-'8 witnessed, on my part, several extraordinary and fruitless efforts to get before the public the theory, of whose truth and importance I was then fully convinced. In 1839, Dr. C. Smith, then of Troy, an able medical lecturer, became a convert to the theory; and showed me, in post mortem dissections, the organs of respiration and circulation. At the close of that year, having carefully corrected and made out copies of my manuscript theory, which I had before written, I sent two to Paris—one to the two brothers, Drs. Edwards, members of the French Institute, and one to my friend, Madame Belloc. I also sent one to Edinburgh, to Dr. Abercrombie. Dr. Milne Edwards soon after wrote a book, in which he made it a point to show that animals could live several minutes without breathing; and Dr.

* Mrs. B. Ogle Taylor, of Washington, formerly Miss Julia Dickinson, of Troy, was thus found dead; and the late Mrs. Cass thus lost her life. "She was seized," says a newspaper account, "in a hot bath, which she had taken soon after eating." She lived an hour, unconscious, and the physician said she died of congestion of the brain. How easily could these highly intelligent ladies have kept themselves from danger, or saved themselves when they felt it approaching, had they known and understood these principles. For two reasons, in case of the failure of the motive power from keeping the body too long in hot water, the blood would be congested in the head. First, the head would not be immersed, and, second, the last blood which the lungs sent forth would go to it.

† What can the Smithsonian Institute do better to carry out the views with which the benevolent Smithson gave his fortune, than thus to teach mankind when life may, by free circulation, be made to confer enjoyment—how it may be inadvertently destroyed—and how it may be restored, when, by drowning or otherwise, it is suspended? Sudden deaths often occur by mal-position. That of the late Secretary Marcy is doubtless an example. After his blood was heated and his circulation quickened, he laid himself down on his back, his head not raised. Attention to the workings of such a piece of apparatus as might be made, would have shown the fatal effects of such a position at such a time.

Frederic Edwards wrote me a short letter of objections to my theory, and adherence to that of Harvey. This letter was copied and answered in my work published in 1846.

About this time, Dr. Aikin, of Baltimore, wrote to me on the subject; and showed, by calculations, that the mere gradual expansion of the water of the blood was not sufficient, of itself, to produce a current as rapid as that of the blood was proved to be, even on the lowest estimate of its velocity. This did not shake my faith in the great fact that circulation was created by respiration. It must be so; for in life, such respiration as produces heat is the inviolable antecedent of circulation, and nothing else is. There was something, then, which remained to be discovered. Again, I placed before me the conditions of the great problem, and set myself intently to its study; and I soon found what I thus sought, and then discerned for the first time that the blood moves, as does the railroad car, by steam. John Bell, my favorite author, had shown that the lungs work *in vacuo*. A great proportion of the blood is water, which, in a vacuum, springs into vapor at 67° , and the temperature of the blood in the lungs is 101° . Its expansion, then, was not merely the gradual increase of bulk by transmitted heat, but also that of instantaneous expansion, by the vaporization of so much of it as is needed; and what expanded water could not do, steam certainly could. At once, a throng of proofs came to my mind. The most apparent of these was the vapor expired in breathing. I recollected how, in former times, the stage horses, driven rapidly into my native village of a winter morning, had clouds of vapor wreathing upward from their nostrils, while the icicles of condensation were hanging below. The nurse, who stands over the dying, holds a mirror before the mouth and nose, and considers that life is only extinct when vapor ceases to be formed. Then came to mind the solution of that great mystery of physiology, why the arteries are empty at death, which so long hindered the discovery finally made by Harvey.

In the state in which chemistry was, even as late as the time of John Bell, the chemical power of the heat produced by respiration at the lungs could not have been understood.

SECTION II.

Publication of the Theory, in 1846, in "A treatise on the Motive Powers which produce the Circulation of the Blood." Its Reception: Critique in the New York "Journal of Medicine," September, 1846. My Reply, in the same Journal, March, 1847.

TO DR. MARCY.—In the years immediately succeeding 1840, (in which year, as you will recollect, I had the honor to receive your countenance and advice respecting my theory,) I was almost exclusively devoted to the revision and enlargement of my historical works; but early in 1846, having deter-

mined on making the tour of the United States, I resolved first to prepare my theory for the press. In the introduction, I remarked, "The house of clay in which the mind dwells must receive a portion of its care; and that which I have bestowed on mine has proceeded on a belief in the truth of the theory herein advocated, as undoubting as that in the laws of gravitation; and when any new fact, or any remark of an author, relating to my theory came under my observation, I noted it down and laid it by with its kindred. About to set out on a long journey, and aware that my field of vision had thus enlarged, I felt it my duty to put together the principal of my remarks, that I might so leave the subject, that, in case anything should prevent my return, it would be in a form equal to the present state in which the theory exists in my own mind."

The time I had spent in devotion to this theory, the many rebuffs I had met in seeking to promulgate it—sometimes, unhappily, affecting my social life—had made painful the duty of publishing it. My historical works had been received with favor; but I believed that, in publishing this, it would be charged against me that I chose a subject unsuited to my sex. I therefore said, in my preface, "This is not so much a subject which I chose, as one which chooses me; and if the Father of Lights has been pleased to reveal to me from the book of his physical truth a sentence before unread, is it for me to suppose that it is for my individual benefit? or is it for you, my reader, to turn away your ear from hearing this truth, and charge its great Author with having ill-chosen his instrument to communicate it?"

As I passed southward on my journey, I left, March, 1846, my manuscript in the hands of Wiley & Putnam, in N. York :* to be published at my expense. During the six months in which I was absent on my travels, my book was published; and the publishers sent copies, as directed by me, to many of my personal friends, and to several physicians. They sent other copies, which procured notices, some of which were favorable, particularly one from the *London Critic*, and others, the reverse. As few copies of the book sold, I was not remunerated for the cost of publication. The copies sent to physicians were mostly unacknowledged—received in cold, if not contemptuous, silence. But my family physician, the worthy and learned Dr. Robbins, to whom I dedicated the work, ever upheld me. He answered my questions, gave me instructions, and showed me post-mortem dissections; and to those who asked him if he believed in my theory, he wisely replied, "Mrs. Willard is right as far as she goes." He knew that I made no pretensions to understand the vast variety of medical subjects not connected with the circulation, and that I never doubted his skill or disputed his prescriptions. An honest man, and a skilful physician, he deserved and had my unfailing confidence. And if, by reason of what I knew, I had prolonged my life, he had the longer kept a good and faithful

* A young physician, whom I paid for correcting the proofs, was not successful in preventing mistakes, especially in regard to numbers.

patient. Lady-friends, to whom I had sent my work, had sometimes referred it to their medical advisers; and thus Dr. Hiester, an eminent physician of Reading, Pa., became a believer. And in the same way, the eminent Dr. Cartwright, then of Natchez, and President of the State Medical Association of Mississippi, came to a knowledge of those principles, which, as we shall hereafter show, he so remarkably elucidated.

In September, 1846, the *New York Journal of Medicine*, then edited by Dr. Charles A. Lee, contained a review or critique on my work, which, if the history of the theory shall hereafter become a matter of special interest, may, with my reply, contained in the March number of 1847, furnish any examiner with the full state of the question at that period.

The learned reviewer showed himself acquainted with the subject as it then stood, and with its history in the past. He held that the heart's action, "the contractile power of the cardiac walls," is the main spring or *primum mobile*, from which the circulating force proceeds, notwithstanding the great discrepancies as to what that force is; and while he objected to my theory, that it did not show any distinct measure of force, he said that, while Borelli estimated the contractive power of the heart at 180,000 pounds, Keill stated it at five ounces, Sir Charles Bell at 51 pounds, Carpenter at 51½, and Hales at 50. He abandoned, however, Harvey's idea that the heart was the only organ of circulation. He believed that it was assisted by the contractile power of the arteries, by the movement of the ribs and chest in respiration, by capillary attraction, muscular contraction in exercise, and several other forces; one of which, the attraction of the venous blood for the pulmonary cells, had been recently pointed out by Dr. Draper. The author did not suppose he was bringing forward any new truths; "but," said he, as an introduction to his account of my theory, "are we not sometimes in danger of forsaking old truths for new theories?"

Of my theory, he says: "The mere statement of it must satisfy our readers that it is wholly untenable. It is well known that heat is generated in every part of the system as well as the lungs. Whenever oxygen and carbon unite, there it is developed; but it is imparted to the *solids* equally with the fluids; it maintains the temperature of the whole body by radiations from the points where it is generated." "It is believed that all those functions of the organism which are necessary for the preservation of life, contribute directly or indirectly to the production of animal heat; so that it is developed at every point at which metamorphosis is occurring, and therefore not merely in the lungs, but in the whole peripheral system."

The writer then observes, that "the heat of the venous blood as it reaches the right side of the heart (according to Davy), varies only two or three degrees from that of the aorta. Granting, then, that the blood receives three degrees in the lungs, it is very evident that the expansion produced by it would be too small to be appreciable. The cause, then, is insufficient to produce the effects." The writer gives me credit for having ingeniously supported

my theory, and then politely bows me out of the department of physiology into my more appropriate sphere of educating girls.

In my reply, this sentence from Cuvier was chosen as a motto: "Respiration is the function essential to the constitution of an animal body; it is that which, in a manner, animalizes it; and we shall see that animals exercise their peculiar functions more completely according as they enjoy greater powers of respiration."* My reasoning was to this effect: "It is in vain to say that cannot be, which is. When two events are so conjoined in nature that one is the only invariable antecedent of the other, then, according to all logic, we are bound to conclude that the first is the physical cause of the second, even though we cannot understand how it should be. Of the circulation, such living respiration as produces heat is the invariable antecedent, and nothing else is. The heart's action, as stated by our reviewer himself, is not therefore respiration, and not the heart's action or anything else, is the cause of the circulation. This argument is upheld by the fact that circulation varies not only as respiration, but as its products digestion, strength, and, according to Cuvier, animal vitality vary. All begin with respiration, end with it, and are as it is. If respiration ceases, restore it before the organism is deranged, and they are all restored. We must conclude, then, that respiration is the cause of circulation, although we could not see how it should be. Much more, when we discern a mighty power, that of expansion, and see how the Almighty has made our frame in reference to its production by caloric—the lungs allowing of heat within them like wet cloth, and the nerves, bones and muscles all made and arranged, so that oxygen shall be brought to them by respiration on the one hand, and carbon by the numerous digestive and circulatory organs on the other.

As to any deficiency of power, my reviewer had omitted to notice that not only the ordinary expansion of the water of the blood by caloric had been assumed, but also its vaporization, or the change of such a portion as was needed into steam, the lungs being *in vacuo*; so that nature here had not failed of her usual abundance. And had not this power been kept in check by the pressure of the surrounding air hindering the perfect vacuum of the lungs, there was reason to fear, rather its excess than its deficiency. As to the reviewer's assertion that heat is generated in every part of the system, and imparted to the *solids* equally with the fluids—that I positively denied, in the name of common sense. For who does not know that, although there may be some heat elaborated in the stomach, and some during the processes by which

* I had just been reading Cuvier, to see whether he believed in the Harveian theory of the circulation. I found he did not. "The circulation vortex," says he, "is sometimes simple, sometimes double and even triple (including that of the vena porta); the rapidity of its movements is often aided by the contraction of a certain fleshy apparatus denominated hearts." Thus showing that my theory gave to the heart all the prominence that was given to it by this great philosopher, who had not, however, advanced any opinion as to the cause of the circulation.

the fluids change to solids, that the great source of heat to the system, is in the fluid blood, and not in solid flesh or bone? Our senses of sight and feeling show us, in the case of blushing, that heat comes and goes with the blood. No one believes that the solid parts of his leg warm the blood as much as it warms them. Finally, it discredited the old theory, that it showed no adequate use for the great primary function of respiration, and its constant attendant, animal heat. Breathing and warmth are not ultimate ends. Man breathes to live; he does not live to breathe. He is warm to live; he does not live merely to be warm. Our theory shows that it is these primary agencies which sustain his being; and it sets forth the manner in which they operate for this end. And thus, while it indicates the wisdom of the Almighty in the formation of the animal frame, it shows itself to be His true interpreter.

SECTION III.

Uses of the Theory—Proofs.—Publication of a Work, in 1849, entitled "Respiration and its Effects, more especially in relation to Asiatic Cholera and other Sinking Diseases."—Examples.

TO DR. MARCY.—The theory of the two chief motive powers which operate at the centre was, we conceive, completed by the addition of steam formed in the vacuum of the lungs, as available to give to the blood its due velocity. We also believe that complete proof *a priori* had been adduced of the fallacy of the theory that the *primum mobile* is in the heart; and, also, that proof *a priori* had been given that it begins at the lungs, and is the product of respiration. It remained to apply this theory to use, and to find proofs *a posteriori*.

Although some of my friends regarded my theory as an *ignis fatuus* which led me into nothing but evil, yet it has enabled me, by plans of exercise, to endure for many years, in-door sedentary labor—and yet enjoy health; and in unusual emergencies, more than once to save my own life and that of others.

In the cold winter of 1835, I took, at Troy, the old summer stage, at midnight, to cross the Green Mountains. I was alone in the large and ill-closed vehicle; the thermometer was sinking as I proceeded on my way, until it had reached 25° below zero, a degree of cold to which I had never before been subjected. When I had traveled alone twenty miles, I found myself in imminent danger of perishing. Ordinary expedients to get warmth were no longer availing; numbness and cold at the vitals were overcoming me; and I knew that to give way to them was to die. I thought of my theory; but I was fearful that I should commit sin if I tampered with the sacred "breath of life." But my necessity was urgent, and I aroused, stood up, and breathed that dense air with violence. It felt for the moment cold to my lungs, but soon came heat

with a rush, and with it pain, as if the whole surface of the throat and lungs were blistered; and my first thought was that I should die, justly punished for my temerity. But soon I was restored to genial warmth; and rejoiced in having successfully made an important physiological experiment.

Afterwards, having been instrumental in relieving a woman who was perishing from having breathed the fumes of charcoal, I was led to reflect that in such cases there was something to be taken away from the lungs, as well as warmth to be added. This woman's extreme coldness, and feeble, fluttering pulse, showed that she was dying for want of right breathing; and in her case there was no doubt that the cause was the same as that of death by drowning. The carbonic acid gas which she had inspired, being heavier than atmospheric air, settled as water in her lungs, and in the same manner prevented the access of oxygen to their living tissues. And hence arose the reflection that the ordinary carbonic acid gas, which is always the residuum of respiration, might, from weakness, settle in the lungs, and thus become the cause of disease and death. The presence of carbonic acid in the lower bronchial tubes and cells, existing in quantities sufficient to prevent the natural combustion by breathing, was brought to my mind in March, 1847, while searching for the cause of an agonizing paroxysm of sick headache. The distressed feelings of obstructed life with which I was tossing and struggling, together with the agonizing pain in the head and pressure on the stomach, might well arise from such a cause. Standing (for position is important) in a full current of air from an open window, I commenced a species of violent artificial breathing, for the purpose of ejecting the supposed heavy gas, and filling my lungs with pure air. This was done by contracting the chest on every side to its smallest possible dimensions, and at the same time throwing out the air violently and from the bottom of the thorax, as if under the operation of an emetic; then alternating by opening the chest to its greatest capacity, and drawing in, by successive inhalations, all the fresh air possible, and pressing it down to the lowest depths of the lungs. This process at first gave such intensity and sharpness to the pain in the head, that it required much resolution to continue it; nevertheless it was persevered in. After a few minutes, the pain diminished, and soon entirely ceased. This was followed by free perspiration, and equalized warmth and circulation. Perfect repose and quiet sleep ensued. Friends, who a short time before had seen a countenance like that of a dying person, and knew how slow was ordinary cure, were astonished, an hour afterwards, to behold, on my awaking, the full glow of restored health.*

On the re-appearance of cholera, during the summer of 1849, my mind was peculiarly affected, from the belief that a false theory of circulation prevailed, although there was a true theory, which, if generally believed, might lead to the knowledge of the cause and cure of this terrific malady; and thus thousands

* One of them, my lamented niece, Jane Porter Lincoln, at my request, immediately wrote an account of the experiment, which is now in my possession.

of lives be saved which would otherwise be lost. This thought almost distracted me; and believing that my sex stood in the way of my theory's being acknowledged, I sometimes wished that it might please God to take me out of the world. Then coming to better thoughts; I cast away despondency as unworthy of me; and determined to proceed to the further investigation and development of the great truth, of which I had, as I believed, been made the unworthy recipient. I studied my theory anew, while I read the most approved works on cholera; and I came to the belief that imperfect respiration, caused by the want of due oxygen in the air, was the principal predisposing cause of the premonitory symptoms; while the death that supervened was often caused by the settling of carbonic acid gas, the residuum of animal combustion, in the lower air-cells of the lungs. The symptoms of the cholera, as treated by the best writers, were full of new proofs of the truth of my theory, especially of its last step, the formation of steam or vapor in the lungs. Without that, the collapse of cholera was a fearful mystery; with it, everything was plain. With a coldness that would collapse the lungs, the bowels must naturally be drawn up (and with dreadful pains) to supply their place. The ghastly change in the face must occur when cold has condensed its arterial vapor. If respiration could restore heat, before any lesions had taken place in the organism, the patient might recover. Then I began rewriting my theory in a work afterwards published, with the title, "Respiration and its Effects, especially in relation to Asiatic Cholera and other Sinking Diseases."

While thus occupied, the debilitating air of the season weighed upon my health and spirits. I had been affected for about three days with what I regarded as the ordinary complaints of the season, when one night, after my family had retired, I found myself suddenly very ill—my symptoms being coldness, debility, and spasmodic pains. I believed myself to be attacked with cholera. I efficiently practised the artificial respiration in fresh air as before described. Gaining strength as I proceeded, I soon found a death-like coldness giving place to genial warmth. Violent exercise, with artificial breathing, was kept up some time, with such rests and full free breathings as nature required; after which, I slept, perspired profusely, and was well in the morning.

This was an occurrence which sunk deep into my mind; and the more so, as I could not speak much of it, for the truth was too improbable to be believed. But the successful issue of this, my first experiment upon the dreaded disease, prepared me to act with boldness and efficiency in a case which occurred in my own house about a week after.

On the 14th of August, 1849, Jane Phayre, an Irish woman in my service, of about twenty-five years of age, having been ill for four days with diarrhœa, was suddenly struck with what the French call cholera *foudroyant*—from fright. Alarmed by unwonted sounds near her window in a basement room, she mounted the window-seat to look out at the top sash, and found her face close to that of a man dying of cholera, who in his death-cramps was brought from a steamboat on a litter, and thus rested upon the pavement.

The cover was lifted from his face, and the sight and the smell struck her with faintness and trembling; and with difficulty she reached her bed. I was called to go to her quickly by Eliza Fagan, who said that Jane was very bad. She had a clay-cold death-look, and a frightful blackness around her eyes. Her face, as I saw it, was livid, pinched in features, and corpse-like, and her pulse but a feeble flutter; and she seemed only to breathe from the top of her lungs. She tried, as she afterwards told me, to say, "I am dying," but her speech was husky and inarticulate. She says her sight and hearing were gone; and while Eliza and I were dragging her out of doors, she could not see the window, and did not feel her feet. We placed her in an upright position, with her back resting against a board-wall, a fresh breeze blowing full in her face. Her senses were now partially restored. I told her to breathe violently, for she must get the bad air out of her lungs and the good air in; and I showed her how she must do it. At first she said, "I can't, for something rises up in the inside." When I told her, sternly, that her life depended on it, and she must, she tried to obey me. At first, it gave her severe headache, but as soon as deep breathing was fairly begun, while I was watching her face with intense anxiety, the color changed from the clay-cold death-look to the full flush of the warm hue of life, and she joyfully exclaimed, "Oh! I feel well!"

When the removal of carbonic acid gas had made way for oxygen to be brought to the yet uninjured lungs, the carbon of the venous blood ignited, the motive power was furnished, the blood was again moved forward into the arterial system, and the dammed up venous current, receiving the suction force, rushed on so violently as at times nearly to produce suffocation; but the struggle was soon over, and the lungs, free both from carbonic acid gas and an unnatural quantity of venous blood, once more received pure air—and to the relieved sufferer respiration became delightful—the circulation passed freely through an unbroken system—and THE CHOLERA WAS CURED.

Was there, in the whole wide world, another person besides myself who would have taken such a living corpse, dragged it out of doors, and set it upright, on feet which could not feel, with the expectation that it might breathe out death, breathe in life, and be restored? The result is a proof, *a posteriori*, that the theory on which the experiment was made is true.

Other cases occurred, where, under different circumstances, cures of cholera were effected. One, as instantaneous, and in some respects as remarkable as that of Jane Phayre, was that of my friend and former pupil, Mrs. Gen. Gould, of Rochester, who sent for me, believing herself to be dying of cholera. I have her letter, which, by permission, is published in my work on Respiration; and also a letter from her physician, Dr. Bloss, of Troy, testifying that her disease was cholera, and that he had little hope of her restoration. This letter is published in the appendix of a report on my theory, read in Buffalo, August 8th, 1851, to a convention of the New York State Association of Teachers.

In my journeying to New York city, to attend their previous convention in August, 1850, an accident obliged me to walk for some distance, in the middle of a hot day. The convention sat in Hope Chapel, which was poorly ventilated; and in the evening, I sat under a large gas-burner. On entering my room at the New-York Hotel, which was on the ground floor, situated where the only air was from a confined, central enclosure, I perceived at the only window a strong smell of fresh paint from the outer walls, so that I was obliged to close it. Being excessively fatigued, I slept heavily—till at early dawn I awaked to find myself in a dying state. Attempting to move my arms, they were like lead by my side—and my breath was but a feeble gasp. Without the knowledge of my theory—my bane, as many of my friends have thought—I should then have had no antidote. But I knew where was the destroying agent, and what was the only means by which I had a chance of removing it; and I used the little strength I had left to breathe deeper, and then to strive for a better position. Long and doubtful was the struggle. It was ten o'clock when, with tottering steps, I got into a carriage, and sought the free fresh air, which enabled me to take a little food. In the evening, I went into the Teacher's Convention, having first ordered from my publisher a sufficient number of my books on Respiration to present one to each member; and then, at my request, a Committee of Investigation was appointed by the convention to report on my theory. They reported favorably to the succeeding convention at Buffalo, which adopted the report, and I published and circulated it. This committee I had been allowed to choose, and it consisted of my friend, Prof. Twiss—the first believer in the theory—and Mr. Fellows, that Professor of Natural Philosophy, who formerly assisted in making my apparatus.

Mr. Fellows carried the report to Buffalo, and when he read it in the convention, editors immediately came to him to request copies for the press. But, by the influence of physicians, they afterwards declined it when offered. It seemed to be the general plan of the regular faculty (in the Eastern, not the Western, States) to put the theory into a condition resembling the algide state of cholera, where it would die of coldness; but, by the aid of Divine Providence, it will, like its author, restore itself by its own inherent vitality—the vitality of immortal truth.

SECTION IV.

Proofs from Dr. Cartwright's Great Experiments on Alligators—Resuscitation of Dr. Ely's Child—Dr. Bowling, Editor of the Nashville Medical Journal, endorses Dr. Washington, who, in that journal, "crushes out" all Opposition to the Theory—Dr. Draper's Acknowledgment of it in New York—Homœopathists—Conclusion.

TO DR. MARCY. Thus, for thirty years, had I maintained, not only without public support, but against discouragements, these great truths, of which I had been allowed for myself such life-giving evidence. But early in December, 1851, Dr. Cartwright, then of New Orleans, announced in a letter to me that he had publicly become my advocate. His name will ever be connected with the theory, on account of the remarkable experiments by which he demonstrated its truth. In the presence of eminent physicians, and other scientific persons, he resuscitated an alligator which had been killed by tying the trachea. After an hour, when neither fire nor the dissecting knife produced signs of pain, Dr. Dowler* laid bare the lungs and the heart. Then a hole was cut in the trachea, below the ligature, and a blow-pipe was introduced, which Professor Forshey* worked with violence. At length, a faint quivering of moving blood was seen in the diaphanous veins of the lungs. The inflating process being continued, the blood next began to run in streams from the lungs into the quiescent heart. The heart began first to quiver, then to pulsate; and signs of life elsewhere appearing, the animal began to move; and soon, strong men could not hold him. Again they bound him to the table, and kept the trachea tied until life was apparently extinct; when, again inflating his lungs, he so thoroughly revived that he became dangerous, snapping at everything, and breaking his cords. For the third time, the trachea was ligatured—the animal expired, and was resuscitated.

Dr. Cartwright says in his letter to me, published in the Boston Medical Journal, January 7th, 1852, "By this resuscitation, your theory of the motive power of the circulation of the blood was established beyond all doubt or dispute." "This vivisection clearly proved that the *primum mobile* of the circulation, and the chief motive powers of the blood, are in the lungs, and not in the heart." Dr. Cartwright mentioned, in the same letter, a case in which his faith in my theory had saved the life of a breathless infant—inducing him to unwonted perseverance in inflating its lungs.

Able opposers to the theory, however, arose in New Orleans, some of whom believed that the resuscitation might have been effected by applications to the nerves. Dr. Cartwright procured, from Gen. Jackson's battle ground, another alligator, which was publicly killed and vivisectioned. The doctor's opponents first tried their means to bring the animal to life, and failed. Then he, by

* These physicians gave certificates of their witnessing and assisting at this memorable experiment, which were published in the *Boston Medical Journal*, February 1852.

artificial respiration, restored the huge reptile as before;—thus proving that artificial respiration could restore suspended animation when nothing else could.

Dr. Ely was one who had opposed and written against the theory. In the meantime, his infant son had cholera, and expired. His medical friends had left him, and a crape was tied to the handle of the front door. Standing by the side of his lifeless babe, Dr. Ely said to himself, "If this theory should be true, I might yet save my child." And profiting by the example of Dr. Cartwright in restoring the dead alligator, he restored his child to life. Remitting his efforts too soon—again the infant ceased to breathe. And again, and yet the third time, the father restored him—when the resuscitation proved complete; and months after, the child was living and in perfect health. Dr. Ely then came promptly forward, and, like a nobly honest man, reported the case as convincing evidence of a truth which he had formerly opposed.*

Whoever wishes to know the history of theories concerning the motive powers of the blood as they then stood, may learn them by looking over files of the Boston Medical and Surgical Journal, edited by Dr. J. V. C. Smith, for the years 1852-'53, and a part of 1854. Dr. Cartwright wrote for it during those years; and, encouraged by his protection, I frequently answered objections, which flowed in from various medical opponents. The objection derived from the foetal circulation, I answered thus, in the Journal, of May, 1852: "The change occurring at birth, so far from falsifying this theory, affords presumptive proof of its truth. When first the air enters the trachea of a new born infant, and animal combustion begins, the inflation of the lungs must open the vessels and vesicles prepared to receive the venous blood. To fill the new-made vacuum, the whole of the blood from the right ventricle rushes through the pulmonary tube, leaving none to go through the *ductus arteriosus*, thus made useless, and henceforth to be abolished. But what is to move the blood from the capillaries of the lungs? The heart's force, insufficient before without aid from the mother's respiration, is now divided, while its work is doubled. A new power must then be generated by the meeting of the air with the carbon of the blood, enkindled by the peculiar functional vitality of the lungs. Without such a power, no perceptible cause exists sufficient to move the blood onward to the left ventricle. But it is moved thither, and with a power which presses down and closes the valve of the *foramen ovale*, thus clearly manifesting that this current exceeds in force that in the right ventricle. Grant that the new function of respiration has furnished a new power, and this astonishing instantaneous metamorphosis from amphibious to mammalian life becomes perfectly intelligible, and the

* Dr. Cartwright also reported the case in a letter which was published in the *Boston Medical Journal*, September, 1852. This resuscitation was more wonderful than those detailed in my published work on "Respiration." All cases of life thus restored are proofs *a posteriori* of the truth of this theory of the arterial circulation.

wisdom of the Creator is fully vindicated; showing that His work has been truly interpreted."

In the *Boston Journal*, of April 21st, 1852, is an article from Dr. Cartwright, entitled "Confirmation of the Willardian, or Important American Discovery," in which the author endeavors to remove what doubtless has been one cause of the delay in acknowledging its truth. "Those members of the profession," he says, "whom science has only *perfumed*, are the most apt 'to look down with proud disdain' on any discovery originating 'with individuals not indoctrinated.' They do not make the proper distinction between selfish quacks who seek publicity 'to line the pocket,' and those 'who, prompted by some mysterious power,' come forward against their interest, and at the risk of their reputation. 'Rather than to contemn and ridicule, it were better to study the manifestations of that mysterious power.' They do not consider that the truth thus brought to light, while they fail to acknowledge it, is affording 'to selfish quackery' a capital to trade on."

To the same effect is the advice given to the profession by Dr. B. F. Washington, of Hannibal, Mo. He says, in the *Nashville Journal of Medicine*, July, 1854, "it is time for us to be acting; the honor of the profession is in danger. The theory of respiration is a truth which will cut its way; and if we do not take it up and teach it, in a few years we may see the mortifying spectacle of the community teaching the profession scientific truths. Quacks have already taken it up, and we have inhalers and air cures of various kinds."*

The first appearance of Dr. Washington as the advocate of my theory was in the *Nashville Journal*, March, 1854; and his fertile genius had there brought a new illustration of its truth. It had, he said, opened his eyes to the explanation of a fact which had puzzled him from his boyhood. "In slaughtering animals, if the trachea was cut, scarcely any hæmorrhage resulted; while, if that was left untouched, full hæmorrhage occurred. By the Willardian theory, the fact is readily susceptible of explanation. The blood, filling the trachea, suspended respiration, and of course the impelling power of the blood was suspended, and the hæmorrhage ceased. The engine could not work without steam. When the trachea was not cut, respiration went on, and kept up the circulation, until the animal was nearly exsanguineous, and the powers of life gave way." This fact was clearly ascertained by Dr. W. K. Bowling, the well-known editor of the *Nashville Journal*, and able professor of the theory and practice of medicine in the university of that place. He sent me the Journal containing this welcome endorsement of my theory from one who was, as Dr. Bowling assured me, "an observer of superior tact and learning," known by his medical compositions as well in

* Good systems of exercise have been made in some respectable institutions for health, openly formed on the principles of this theory. Such is that by Dr. Hamilton, of Saratoga.

Europe as America. Since that time (March, 1854), that Journal, though not excluding articles which oppose, has been understood to be in favor of the theory. Dr. Washington has written repeatedly, answering all objections ;* and he has, in the Journal (as I have been assured by one of the Editors), "crushed out all that would take up his glove, and is left in undisputed possession of the field—looking in vain for an opponent."

In the meantime, in 1856, Dr. J. N. Draper, Professor of Chemistry and Physiology in the University of New-York, in an elaborate work on "Human Physiology," has agreed that Harvey's theory of the paramount power of the heart's action in the circulation must be abandoned ; and that to respiration must be assigned "the great duty of originating the blood's circulation."†

Dr. Washington has not only defended me in every important position which I have taken, and added new illustrations—but he has made the theory available to showing new proofs of the wisdom of God in the creation of man. Thus—steam is formed in the vacuum of the lungs at the low temperature of 67° , while, if there were no vacuum, 212° of heat would be required to produce it,—an impossible quantity, since it would coagulate the albumen of the blood. But form the vacuum, and the boiling of the blood with any degree of heat less than 101° could not cause any such disaster, while the steam going off from the lungs through the arterial system to the capillaries, gradually condenses, warming the body by giving off its latent heat ; and the latent heat of vapor is the same however it is formed, and is always $1,114^{\circ}$. What divine wisdom and economy are thus displayed !

~ Homœopathy has, we believe, never found any difficulty in receiving this theory. We know that, at one of its conventions held in Providence, it was ably supported ; and Dr. Marcy, whom I have the honor to address, was, as we have seen, one of its earliest defenders. He has never, whether allopathist or homœopathist, been known to hesitate when his own mind brought him clear conclusions ;—the distinguishing mark, according to Dugald Stewart, of intrepidity of character.

With profound respect,

EMMA WILLARD.

* When the time shall come that, the truth of my discovery being no longer denied, its originality shall be contested, it will be a significant fact that, in the *Nashville Journal*, of September, 1854, is an article against it from a physician signing himself "Justicia," which he thus heads, "The Willardian Notion." In evil report, it was indisputably mine. This article also shows, that the Harveian theory is still maintained by the opposers of mine.

† See Draper's Physiology, p. 142.

1911

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